Journal of the Oxford University Junior Scientific Club

JUNE, 1911

The Growth of a Crystal

BEING THE

EIGHTEENTH ROBERT BOYLE LECTURE

DELIVERED BEFORE

THE OXFORD UNIVERSITY JUNIOR SCIENTIFIC CLUB

On the 20th of May, 1911

BY

HENRY A. MIERS, M.A., D.Sc. (Oxon.), F.R.S.

PRINCIPAL OF LONDON UNIVERSITY

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THE GROWTH OF A CRYSTAL

When this date was fixed by your Secretary for the delivery of the Boyle Lecture, I discovered that it happened to be the fifteenth anniversary of the very day on which I was first called upon to address a general audience in Oxford. On May 20, 1896, I delivered an inaugural lecture as Waynflete Professor of Mineralogy; when I look back upon the happy years spent here in teaching and studying a science which is dear to me, I feel that the present lecture should be an opportunity for expressing gratitude for those peaceful years, not unmingled with regret that they led to no such worthy achievement on my part as might have brought great credit to the University and so have repaid something of the debt which I owe to her.

Looking back from the busier world of London, it is easy to see how ideal are the conditions under which an Oxford Professor conducts his work; especially if his subject be one which does not overwhelm him with students who pursue it only for the purpose of passing an examination. Those who are attracted to his Laboratory probably come because they have some natural taste for the subject; he finds it a pleasure to devote his time to them; while his vacations and the conditions of Oxford life give him unique opportunities for his own researches.

It is true that those who have most leisure not infrequently waste most time. It is true also that the

custom of Oxford is to burden her students and scholars in addition to their teaching, with the conduct of affairs which could be managed as well by persons specially appointed for the purpose. Still it is also certain that to those who enter into the genius of the place, and are animated by the spirit of Learning, Oxford is prodigal of opportunity, and enables them to live the Academic Life in a way which is scarcely possible elsewhere.

I know that the doors of the University are being opened to all the newer studies, and that many a student spends most of his time in acquiring the useful knowledge that is to equip him for his profession and for the direct purpose of that profession; knowledge which is to fit him to become lawyer, doctor, minister, engineer, or teacher; yet an Oxford Professor may always maintain the pursuit of Learning for its own sake and keep this purpose before his students even in their most technical work.

Now Mineralogy is one of those sciences whose practical applications are clear; it is necessary to the miner and the engineer; indeed, it sprang from their needs; even Crystallography (that is, the study of Crystals), which has always hitherto, though with little reason, been treated for University purposes as a branch of Mineralogy, has also become part of the necessary equipment of the practical chemist and geologist; but both Sciences are, in their general aspects, very far removed from the turmoil of practical life; it is with these aspects that I would fain deal, and especially in relation to the study of crystals.

In this connexion, a passage which I quoted from GOETHE fifteen years ago will bear repetition: '*There is a flavour,*' he says, '*of the Monk or of the old Bachelor*

about Crystallography and therefore it is self-sufficient. Practical application in life it has none; its rarest objects, the crystallized precious stones, have to be cut and polished before we can adorn our ladies with them.'

In fact, this lecture, which I was constrained to prepare during a brief holiday in Italy, was written in the midst of surroundings where it was easier to think of Science as cultivated in the quiet of the Laboratory, rather than in the restless scenes of its practical applications, although I am familiar with both. Writing at a window overlooking the mediaeval town and ancient walls of Perugia, with the view of peaceful Assisi and the snowy cap of Monte Subasio across the plain, it was easier to recall the hours of quiet toil and reflection spent in one's Laboratory at Oxford than visits to Mining Camp, or Metallurgical Works.

Accordingly when, under these circumstances, I set about choosing a subject for the present occasion, it occurred to me that I might be allowed to take up my discourse where I laid it down in 1896 and in some sense to continue and conclude the remarks which I then made to the University; and, considering how recently I have left you, I might regard this as my farewell address corresponding to the opening lecture which I then delivered.

In that address, I recollect, I began with an inquiry into the resemblances and differences between minerals and other objects of nature in respect of their beauty, especially as regards the beauty of form which is so specially characteristic of minerals. Some minerals, indeed, are found in delicate fronds and leaflets, and in mossy tufts, which, in their form and texture, in their sheen and lustre, so closely resemble plants that

they are often mistaken for them. [Those who heard Ruskin's lectures will remember the delight with which he described these beauties of the mineral kingdom and the affection which he felt for them.] I pointed out, I remember, that these delicate forms are, like the frost patterns on a window-pane, really expressions of the crystalline shape and symmetry of the mineral; each mineral consists of crystals, and therefore has its own peculiar crystalline shape which is one of its inherent properties; moreover, this persists unchanging through the ages, and under all conditions, and is in no way dependent upon the environment in which the mineral is situated.

The shape of a crystal does not depend like that of the animal or the plant upon the life which its forefathers have led or the conditions under which they have grown.

There are indeed other features in which a mineral or any crystal may resemble a living thing in a way even more surprising than in its form. Two of the most remarkable are these: it grows out of a solution as though it were alive; and, if it is wounded or broken, it heals itself and replaces the missing part just as a living organism may do. But, ~~as I pointed~~ out, there is this radical difference. The crystal is not responsive to the change of its surroundings; its form is not the result of external forces; it does not adapt itself to its environment; it is not undergoing any progressive evolution; but remains fixed and unchanging. Its form is the expression of its permanent composition, and so far as we know has always been the same. Indeed, until it has been converted into its constituent elements and destroyed, it is in a sense not only unchanging, but imperishable. For,

take a crystal and break or dissolve it away until only the tiniest fragment remains ; that fragment (e. g. sugar), though it may be only an invisible speck, will, if immersed in the appropriate solution, continue to grow again and will once more assume the form of a perfect crystal. Neither does it make any difference if the crystal fragment has been kept for years or even centuries; it will, when supplied with nourishment from the appropriate solution, heal itself and continue to grow as though the process had never been suspended. In a sense it is immortal, for, if not destroyed, it never loses the mysterious power of growth, and is therefore more imperishable than any seed or germ of life.

The main conclusion of my lecture was that, having been led by analogy to compare the growing crystal with the growing plant, one finds the growth and life of the crystal to be totally unlike the growth and life of any organism. Its life is only an unchanging persistence without the display of any struggle for existence, any movement, any adaptation, or response to environment; its growth is only the addition of new material on the surface of the old, without assimilation, so that the crystal remains uniform and increases in size; there is no distinction of any one part from any other; the crystal grows, therefore, without the development of any organs.

One appears to be left with the conclusion that the crystal should be regarded rather as a type of death than of immortality.

Schopenhauer expressed this idea when he said: '*The crystal has only one manifestation of life, crystallization, which afterwards has its fully adequate and exhaustive*

expression in the rigid form—the corpse of that momentary life.'

This, however, is not the conclusion which I would have you draw from the facts; and I shall ask you on the present occasion to accompany me in pursuing a little further the inquiry into what we may call the vitality of a crystal and the manner in which it is displayed; although this vitality is not to be confounded in any way with that of a living plant or creature.

It is, I hope, not unfitting that the subject of crystal growth should be dealt with in a BOYLE Lecture, for ROBERT BOYLE was himself one of the first to treat it in a scientific spirit. He was the exact contemporary of NICOLAS STENO, the famous Danish physician who laid the foundation of modern crystallography; and BOYLE, in his treatise on the origin and virtues of gems, was the first to express the conviction which was almost simultaneously expressed by STENO—that gems must have solidified from the liquid state. One of his reasons for thinking so was their crystalline form, which resembles that of salts which crystallize from solutions. Let me quote his exact words: '*The origin assigned to gems may be countenanced by the external figuration of divers of them. For we plainly see that the corpuscles of nitre, allum, vitriol, and even common salt, being suffered to coagulate in the liquors they swam in before, will convene into crystals of curious and determinate shapes.*' And then he points out that when a salt such as nitre crystallizes in a vessel it is only where it is free to grow in the liquid, and away from contact with the sides of the vessel, that it can acquire this shape. These crystals (he says), '*having a fluid ambient to shoot in, will have those parts of their bodies that are contiguous to the liquor curiously*

formed into such prismatical shapes as are proper to nitre.'

When BOYLE wrote these words he had not seen the work of STENO, which had just been published.

I may, therefore, claim that he was the first person to deal in a scientific spirit with the subject which I have chosen for this lecture, the 'Growth of a Crystal.'

Three years before I came back to Oxford—on May 16th, 1893, the subject of crystal structure had been treated in the second BOYLE Lecture in a most original and masterly manner by no less a person than LORD KELVIN. I well remember that Lecture, at which I had the good fortune to be present; and to one who was already interested in crystals it was a wonderfully illuminating and inspiring address.

On that occasion my venerable predecessor and teacher, Professor STORY MASKELYNE, conducted LORD KELVIN to the lecture table.[1]

At that time, however, there was no laboratory in Oxford for mineralogical and crystallographical research; the Professor only had a lecture-room, he did not reside in Oxford, and his scientific work was necessarily carried on elsewhere. To-day I am more fortunate in being able to draw upon the resources of the well-equipped laboratory of my successor, and former pupil, Professor BOWMAN, and upon his still more valuable personal assistance and that of Mr. BARKER; so that, though I only have to deal with ideas that are simple compared with those which issued with fiery vigour from the fertile brain of the BOYLE Lecturer of 1893, I have a

[1] It is with the deepest regret that I add that my old friend and revered teacher died on the very day on which these words were spoken.—H. A. M.

better opportunity of showing to an Oxford audience to-day the actual things of which I am speaking, and may help to make my meaning clear to those who cannot know much from personal experience concerning the growth of crystals.[1] When I delivered my own inaugural lecture I had no means of making visible to an audience the astonishing features of crystal growth. Beautiful effects may easily be witnessed by anyone with a few drops of common solution and a magnifying glass, yet I believe that they are witnessed by comparatively few persons. according to the

I have alluded to laws of crystalline structure, to which Lord Kelvin had directed attention: these are the laws of geometrical arrangement which prove crystals are ~~to be~~ constructed in an entirely different manner from the living plants which they may so closely resemble; these laws, however, were not established by observations or experiments upon the growth of crystals. They were the result of a century of patient measurements of the external shape of countless crystals; more than a century of accurate determinations of what happens when heat and light are transmitted through them and of numberless other experiments made by physicists; and, added to this, the labours of mathematicians who studied the manner in which solid particles could be arranged so as to correspond to the geometrical and physical proportions thus determined by experiment.

But all these were experiments and reasoning upon matter which appears to be as nearly as possible inert; they entirely ignore the power of growth possessed by

[1] This lecture was beautifully illustrated by excellent slides showing the actual *growth* of crystals.—Ed.

crystals; indeed, no such power is contemplated by the ordinary theories of crystal structure or could be predicted from them.

So far, then, we may regard the comparison of crystals with plants and all the fanciful ideas concerning their connexion with the origin of life which were suggested by that comparison as only one more example of the dangers of reasoning from analogy. The recollections that I retain of the allusions to that process—in text-books of logic and treatises on Science alike—give me the impression that examples of reasoning from analogy are generally quoted only as instances of its danger and futility. That, indeed, might seem to be the conclusion of my own inaugural lecture; for, when we come to examine the growth and structure of crystals, so far from finding that there is any real likeness to the life and structure of the plants which they resemble, we find nothing but a profound difference.

That, however, is not my real opinion, and was not my opinion fifteen years ago. On the contrary, I have the greatest belief in analogy as one of the most useful guides to discovery, and as the means by which in practice new lines of investigation are most frequently opened and new hypotheses suggested. In fact, I think that most of the advances which are made in science, and especially in scientific theory, have been made with the help of analogies.

If an explanation of any fact in Nature consists in correlating it with some apparently distinct fact, and showing that the two have a common cause, or are connected in a definite manner, how often has the explanation of a new occurrence been suggested by the analogy of some other known occurrence which is

brought to mind by the memory; not by a conscious effort of the reason, but by the recollection of a resemblance. Do not some of the standard methods which are familiar in the descriptions or criticisms of scientific discovery, such as '*reasoning from the known to the unknown*', '*the adoption of a working hypothesis*', '*the scientific use of the imagination*', often resolve themselves on analysis into the simple process of being struck by an analogy and being led by it to adopt an explanation or to try an experiment suggested by it?

I daresay that scientific discoverers are ashamed to confess that they may have been led to a theory by a superficial analogy just as they are ashamed to confess that they have hit upon a discovery or an invention by chance, and have found one thing when they were looking for another. But there is no need to be ashamed, for the discoveries only come to those who have the eyes to see, or the knowledge which enables them to remember a resemblance, and who have further the intellectual power to make use of it. Science grows on the acquisition of new knowledge and we must not hesitate to grasp it where we can. There is a danger lest the formulation of the methods of science may deter the inquisitive student from seeking knowledge wherever it is to be found, and make him believe that it is only by the orthodox processes of reasoning based upon a life-time of training that he is to discover anything new. Rather let him be encouraged to seek any resemblance or analogy that may point a way in the gloom.

It is true that in the past the argument from analogy has often proved dangerous when, because the things possess certain attributes in common, it was inferred that they are alike in other respects. Yet even here

it may prove useful. Newton only asserted that the diamond is inflammable because it resembles other inflammable substances in possessing a high refractive power; and yet he turned out to be right, although his analogy was wrong. Moreover, it was the analogy which prompted the experiment. To detect an analogy, to test it, and to find that it cannot be maintained, may be as useful an addition to knowledge as the establishment of a real causal relationship: it has had the effect of setting the worker on to new experiments or observations, and every such step is necessarily an advance. The conscientious search for one thing almost invariably leads to the discovery of another; and, even if it does not, who shall say that it may not be as important and useful, say, to establish a difference as to prove the resemblance which has been suspected?

If, however, reasoning from an analogy may be fallacious when inferences are drawn from the fact that two things possess the same attributes in common, it is, I think, a safer guide when it is used to suggest a new hypothesis; a corpuscular theory of light may be suggested by the analogy between the reflexion of light and the rebounding of elastic bodies; an undulatory theory by the analogy of a wave propagated along a string or on the surface of water. The one theory may be found to fit the facts and the other may be condemned; but without some analogy as a guide how are such theories to be devised? What we require in science are continual stimuli to start us on new experiments, new observations, and new ideas, and prevent us, especially those who are teachers, from constantly repeating the old ones.

I shall be satisfied if the present lecture is regarded

as a plea for the use of reasoning from analogy, and as an illustration of its value.

Let me, then, after this exhortation, return to my problem of the growth of crystals, and show how I still think that we may be guided by analogy in seeking to understand it.

So far from regarding the growth of a crystal as of no further importance after it has been proved to be quite different from the growth of a plant, we must still think of it as one of the most interesting and mysterious events, and the one through which we may hope to get the clearest insight into the nature of the crystal itself.

For, after all, when after making physical experiments upon the solid crystal and studying its action upon heat and light we are led to speculate upon the manner in which it is constructed, we must not forget that it grew, and that the material was laid down under conditions which we can only understand by studying what happens on the surface as it grows.

LORD CURZON devoted his ROMANES Lecture to the consideration of Frontiers, and explained their interest and importance in the growth of nations. In the study of Science also nothing is more interesting and important than frontier problems. In two senses is this true. The problems which lie on the borderland between two sciences are the most fruitful of all, because they throw light upon each science, and, by bringing into harmony things that were previously distinct and separate, lead to an immediate extension of knowledge.

End

And also in a more restricted sense. You will find, I think, that the scenes of the most interesting events in Nature are generally those places where different things

come into contact and where there is consequently stir and action; where two substances meet to form a new chemical compound; where two bodies touch and react upon each other; at the surface of a solid or a liquid; these are the regions in which events are taking place that we can study and measure with the prospect of discovery.

Impressed by the analogy between the growth of crystals and of living things, I have always felt that, for a proper understanding of the things themselves, the study of their growth is as important for the one as for the other; that to obtain this understanding it is necessary to study what is happening on the surface of the growing crystal where the advancing solid is in actual contact with the solidifying liquid. If you wish to understand a plant or an animal it is not enough to study dried specimens or specimens in spirits, but to watch the living organisms growing under natural conditions; and in the same way it is surely worth while to study the growing crystal surrounded by the solution which feeds it, and even to make under these conditions the measurements and observations which are usually made upon crystals only after they have been taken out of the solution and have ceased to grow.

In the case of living things, we know that growth takes place by internal processes and not only by new material added on the surface. How much easier, then, should it be to study the growth of a crystal when we have found that it is only a surface activity, and does not involve internal changes?

As I have already said, when I came to Oxford there was no laboratory of Mineralogy nor any apparatus for research, but I brought with me a piece of apparatus

There is an apparatus now which

which I had constructed a few years previously for this exact purpose; to measure the angles of crystals while they are growing in the solution and ~~to~~ ascertain whether any changes take place in those angles, in the hope of getting some insight into the nature of the surface and what I have called the frontier problem. Many days and also nights had I spent with this apparatus in the absorbing pursuit of measuring growing crystals, watching the curious changes that take place in the position of their facets, excited by the knowledge that I was looking at things that had certainly not been seen before, and by the expectation of what they might disclose.

I need not weary this audience with any description of these experiments, which doubtless seem more interesting and important to their author than to anyone else. But I wish to draw your attention to the result that came out of them. I ~~found that~~ also it was is possible with the same apparatus to measure the refractive power of the liquid in absolute contact with the growing crystal and from this to calculate the exact strength of the solution at that spot. It ~~was~~ is thus possible to prove that the solution in contact with the crystal is rather stronger than at a very short distance from it, and to know exactly how much stronger. In other words, while a crystal of, say, alum is growing, the liquid in contact with it contains more particles of alum and less particles of water, or is richer in alum, than the liquid at a short distance from it. I will ask you to bear this in mind in what follows. There is another curious fact connected with this subject. Crystals of nitrate of soda have almost exactly the same shape and almost the same physical properties as crystals of the common mineral

calcite, which, in its purest and most perfect form of transparent glassy crystals, is known as Iceland spar. Now, when a perfectly clean crystal of Iceland spar is immersed in a strong solution of nitrate of soda, although it is not dissolved by the liquid itself and therefore cannot crystallize out of it, the Iceland spar actually continues to grow, and becomes enveloped by the nitrate of soda so as to form what is apparently a single crystal.

It appears, therefore, highly probable that a crystal of nitrate of soda and a crystal of Iceland spar behave alike in this respect when placed in a strong solution of the nitrate; each draws to itself the liquid nitrate in the solution, then draws it out of the solution in the solid state, and further arranges the particles upon its own surface in a perfectly regular manner, so that the arrangement of the particles in the shell of nitrate is the same as the arrangement of the particles in the spar which it surrounds; just as a bricklayer sets upon the rising wall new bricks arranged in the same way as those which he has already laid. I remember that on Lord Kelvin's last visit to Oxford, shortly before his death, mindful of his Boyle Lecture, I showed him, in company with my pupil, Mr. Barker, this beautiful experiment, with which he was at that time not familiar, and I shall never forget the interest and enthusiasm with which he witnessed the beautifully regular and instantaneous growth of the nitrate crystals. He always was as enthusiastic and inquiring as a boy, and these characteristics were exhibited on that occasion in his old age. It is an experiment which sets one thinking, and I have no doubt that, if Lord Kelvin, even at that advanced age, had set his mind to consider it, he would

have been able to deduce far more than it has yet suggested to those who have witnessed it.[1]

Some two years or so before the time of which I am speaking, Mr. BARKER had at my suggestion made an exhaustive study of a great number of different substances in order to ascertain which of them behave towards one another like nitrate of soda and calcite, and why they do so, and he has really discovered the secret. When two substances like nitrate of soda and calcite have nearly the same shape and resemble one another closely in their physical properties, the geometrical laws of crystalline structure of which I have already spoken make it certain that they consist of particles arranged in the same way. We do not know what these particles are or what is their shape or size, but we may be sure that they are arranged in the same way. I am perhaps using the word particles in a loose sense, for they might be hollow cells or they might be the space occupied by moving particles, or anything else, but whatever they may be it is pretty certain that they are arranged in the same way.

Well, Mr. BARKER has proved that, if two crystals grow together like nitrate of soda and calcite, their particles are not only arranged in the same way, but they must be of the same size, or at any rate occupy the same space. In more scientific language, the two crystals not only have the same molecular structure, but the same molecular volume. It is, therefore, mainly a question of fitting together, and, if the two structures do not fit, they cannot grow together, like nitrate of soda and Iceland spar, as a continuous crystal.

[1] The experiment was then shown, exactly as it was shown to Lord Kelvin.—ED.

Let me illustrate by a suggestive comparison. The bee's cell is one of the most remarkable and symmetrical structures in nature. Its regularity is probably due to the fact that bees of the same size are, in making it, so closely crowded together; there is one bee's head in each cell, and therefore you may say that the arrangement of the bees is the same as that of the cells. For example, if you place in each cell a ball which exactly fits it and then take away the cells, you have an arrangement of balls which is the same as that of the cells, each being in contact with six neighbours. It may be called a hexagonal arrangement. You have only to push together a number of balls on a table, and they will fall into this arrangement. It was a contemporary and associate of BOYLE, and an Oxford man, Dr. ROBERT HOOKE, who pointed out that with balls piled together in this way you can build up the shapes of crystals, and that, for example, a pyramid of cannon-balls stacked together in the manner that I have just shown, has the shape and angles of an alum crystal. Now, if two such arrangements are to fit together, they must be of the same size, whether they consist of bees, or cells, or balls, or molecules. In the same way Mr. BARKER has proved that Iceland spar and nitrate of soda must consist of materials which are not only arranged in the same way, but are of the same size, although we do not know in the least what that material is, nor what its actual size may be.

But this, of course, is not the end of the whole mystery: for not only do two such structures fit together, as might be expected, but each has the remarkable property of making the other crystallize and grow, and that means, as I have just explained, that

it draws the other out of the solution where it is liquid into the crystal where it becomes part of a solid structure and lays it down in the exact position in which it fits, just as one bee's cell is added to another in the growth of the comb. We have advanced a step, but only one step, towards the better understanding of the mystery, and I would beg you to note how we are continually led on by analogies which may be quite false, but which are at any rate fruitful.

Now let me pass to another series of researches which were conducted by Miss Isaac and myself for a few years before I left Oxford and have since been carried on by her with conspicuous success. Still experimenting with the same apparatus, and endeavouring to trace how a solution changes in strength while it is crystallizing, we came across some curious and unexpected results. It is, of course, well known that if a crystal, say of alum, is placed in a weak solution of alum it is dissolved, and only has the effect of making the solution stronger, but that at last a stage is reached at which the solution becomes 'saturated' and can dissolve no more, just as a stage is reached at which a soaking sponge will hold no more water. At this point a crystal, if put into it, remains unchanged. But it is quite easy to make the solution still stronger, not by adding alum to it, but by taking water away from it by evaporation: it then becomes oversaturated or 'supersaturated', and now a crystal of alum dipped into the liquid will at once begin to grow and to make the solution weaker. In fact, this is an unfailing test by which we can tell whether a solution is saturated or supersaturated; and, more than this, until a bit of solid crystal gets into it the liquid does not crystallize. Keep

it in a closed vessel so that no speck of alum can fall into it, and it will remain liquid for weeks or years or as long as you please. But let the smallest possible grain of an alum crystal fall into it, and crystallization will be started; inoculate it with an invisible germ of alum dust, such as must be flying about in the air of any room where dry alum is or has been kept, and you will see the life and growth of a crystal begin when that germ is introduced. This is the most extraordinarily sensitive test, and one that can easily be applied.

In many of our experiments we found that during the first day when we were working with some new substance it would not crystallize from an exposed solution; but on the second day, when the air of the laboratory had become impregnated with crystal germs, an exposed solution would begin to crystallize at once.

Dr. Tutton, one of the most accomplished investigators of crystals, whose refined and beautiful researches were for many years carried on in Oxford, has fully described these effects in his two books on crystals just published, and confirms them from his own experience.

Now, Miss Isaac and I have found in the course of our researches that, as the solution becomes stronger and stronger—say, by the evaporation of some of its water, it continues to be in the ordinary state of supersaturation in which it does not crystallize save by inoculation with a germ of solid alum crystal, but at last it suddenly reaches a condition in which it can crystallize spontaneously, and at this moment it is enough to stir or shake the solution, and you will at once witness the birth of thousands of tiny crystals which appear as a cloud in the liquid and begin to grow rapidly.

A very easy way in which to make this experiment is

to dip a clean needle into a drop of evaporating solution on a glass plate and to scratch the glass on which the drop lies: for a time nothing happens, and then suddenly, as the liquid passes into the new condition, a chain of tiny crystals appears along the line of scratch.

That this suspended crystallization has a fixed limit was suspected, and had been predicted by the German chemist Ostwald, but could never be proved until we made our experiments, and it was Mr. Hartley who first helped us to interpret our results. He has subsequently, with his pupils, made a number of investigations on the same subject.

I can show you the two conditions in one and the same drop by using a solution of common potassium bichromate. Crystals begin to grow at the edge of the drop where the liquid first becomes sufficiently strong, and continue to grow slowly in the evaporating liquid which is only slightly supersaturated. But in a few moments other parts of the drop which are thinner become so strongly supersaturated that they begin to crystallize spontaneously; and there you can witness the birth of new crystals which grow rapidly in all directions, because they are growing in a solution which is much stronger than that in which the first crystals are growing slowly.

Does not all this set one thinking? What is taking place we cannot tell, but we can only think of it as in some way analogous to the birth of a cloud; and in both instances we have to picture to ourselves minute invisible particles, of whose shape and size we know nothing, coming together and coalescing till they grow into a drop or a crystal that we can see. But why they do not begin to coalesce as soon as the liquid is super-

saturated it is difficult to say. We have to conceive the alum solution as made up of moving particles of alum and of water, and it may be that the particles are constantly coalescing into minute groups, but as rapidly being broken up again, until a moment arrives at which the alum particles are sufficiently dense to cohere permanently; but how they attract one another and arrange themselves into the wonderful structure which makes a crystal, of this we are entirely ignorant. The question brings us back again to our initial mystery, how does the crystal actually grow?

But this is not all. I have said that all solutions seem to behave in the same way, and among them nitrate of soda, which we have already seen growing in perfect regularity on Iceland spar. It appears, however, from experiments made by Mr. BARKER, Miss ISAAC, M. CHEVALIER (who was another of my pupils), and myself, that Iceland spar behaves in this respect also exactly like nitrate of soda. In a solution which is supersaturated, but is not strong enough to crystallize spontaneously, not only will inoculation with a crystal of nitrate produce instant crystallization; but inoculation with a crystal of Iceland spar produces the same result. So we have a still more convincing proof of what I suggested a short time ago, that two crystals, which have structures so nearly identical that they can fit together, possess also the power of drawing each other from the liquid state into the solid form of a crystal. Whatever it is which conditions the fitting together of two structures must then also confer upon them this extraordinary power of making each other grow.

If I had more time, I should like to give an account of some of the more important discoveries that have been

made about crystals during the last fifteen years, for they would make it easier to understand the present state of our knowledge concerning them. I will only refer to two: one is an experimental fact, and the other is a theoretical speculation, and both are connected with the subject that I have been discussing.

It was discovered shortly before that time, and has been found by many experiments since, that there are certain substances which are in a real sense crystals, although they are liquid; that is to say, they affect light in its passage through them just as solid crystals do.

These extraordinary substances, which had been investigated by Professor LEHMANN, were first shown in England by Mr. BOWMAN and Mr. HARTLEY, who were then working in my laboratory, at a conversazione at the Royal Society, not long after their discovery, and I can well remember the interest with which they were witnessed by Sir GEORGE STOKES and others. We can only picture these liquids as consisting of particles which, while they are free to move in all directions, always continue to face the same way, like a group of dancers who in all their evolutions continue to face the audience, instead of turning as they move. When the mechanism which renders possible this remarkable behaviour is better understood, we may be sure that it will bring about a better understanding of the manner in which a solid crystal is constructed. The interesting thing about it is that here at any rate the particles are in violent movement instead of being comparatively stationary, as they are in a solid crystal.

One is naturally led to imagine that before any solution begins to crystallize in the solid form it passes into this liquid state, and that the particles have begun to set

themselves and all to face the same way before they begin to cohere and to build themselves into a solid. But so far as I know there is no evidence in favour of this suggestion—a solution before solid crystals begin to appear does not behave like a liquid crystal, but remains an ordinary solution up to the last moment when new crystals are born in it or are started by inoculation with a crystal germ.

The other discovery, which is in the nature of a speculation, is that of another person whom I am proud to reckon among my former pupils, namely, Professor Pope of Cambridge, working in conjunction with Mr. Barlow. Mr. Barlow had already been referred to by Lord Kelvin in his Boyle Lecture as the author of ingenious researches upon the various ways in which materials can be packed together, and the different arrangements and structures which result from this packing.

These two workers have now propounded a theory according to which, if the various atoms which constitute a substance are represented by spheres whose sizes represent the valency of the atoms, and if these spheres are packed together as closely as they will go, the resulting structure will represent very nearly the structure of the crystal; and so it may be possible for the first time from a knowledge of the chemical constitution of a substance to predict the structure of its crystals and therefore the form in which it will crystallize. You remember the bee's cell arrangement and the similar arrangement of balls got by placing a ball in each cell and then removing the cells. Another way of getting the same arrangement is to place a number of equal balls on a table and to squeeze them together until they

are packed as closely as possible. This arrangement of closest packing, the arrangement of a pyramid of cannon-balls, is precisely the same as before, the one in which each ball on the table is in contact with six others. According to Pope and Barlow the atoms in a crystal simply pack themselves together as closely as possible, but instead of being equal in size they have generally to be represented as of different sizes according to their valencies. If we imagine the coalescence of atoms to form a crystal to be due to their mutual attraction, it is very reasonable to suppose that they will get as close together as is possible, and therefore that the ways of close packing are the ways of crystal structure. The theory therefore suggests a reason for the growth as well as for the shape of a crystal. I may remind you that the bee's cell itself, which is in the world of life the thing that most nearly resembles crystalline structure, is due to this same principle of close packing; for in their efforts to get as closely together as possible the bees are constrained to get into the hexagonal arrangement. The bees crowd their heads together and to each bee's head corresponds one cell.

On the other hand, Professor Sollas has brought forward some most suggestive and convincing speculations concerning certain crystals which are based upon the principle of open, and not close, packing. His model of silver iodide, for example, is well known in Oxford.

I have mentioned these recent contributions to science, not only with the object of indicating that our knowledge of crystals is steadily increasing, but also in order to point out that little has yet been done to explain the mysteries of their growth. All that has been effected up to the present is an attempt to explain how they are

constructed, not the process by which the construction takes place. It is as though we were to analyse the form and structure of animals and plants and never to watch them as they grow, but only to study them from fossils or from museum specimens. And I believe the reason to be this. All the speculations concerning crystals persist in regarding the particles of which they consist as fixed and immovable: the theories are all statical. And yet we know that the particles of matter, whatever they may be, are really in lively movement. Is it not possible that in order to get a correct understanding of the growth of a crystal we should take account not only of the positions, but of the movements, of its particles? Without some knowledge of these we are not able to approach the problem, or to ascertain how a crystal either of nitrate of soda or of Iceland spar draws the nitrate of soda out of the solution and makes it grow into a solid.

Remember that when we say we know how the particles of a crystal are arranged we really know nothing about their nature, and can only represent them by spheres, or solid figures, or cells, or even points, in order to get a representation of their arrangement. But we might arrange in the same way a number of bodies each of which is whirling in a fixed orbit, like a planet, about the corresponding point, or vibrating about it like the prong of a tuning-fork, or pulsating like a breathing animal; and so far from the arrangement being independent of the movements it may be due to them.

If I may seek another analogy, let me take a group of figure skaters; their centre remains fixed at the orange, maybe, about which their figures are executed, but the

group of skaters is at one moment extended when they circle out to their furthest sweep, and at another moment concentrated when they converge to the centre; and this alternating expansion and compression occurs in a regular rhythm.

Imagine a pond covered by a number of such groups of skaters; the manner in which they will fit in, and have to arrange themselves, will depend both upon the dimensions and the rhythm of their curves, and they may even interlace and become part of one great figure system covering the whole pond. May not the growth of a crystal be something of this sort?

All the devices which I have quoted for picturing to ourselves the architecture of a crystal I would regard as merely models representing something that may be really quite different. But I venture to suggest that the time has come when we should make use of moving and not stationary models.

One need not go further than a spinning top for an illustration of stability due to movement, and there is nothing unreasonable in the suggestion that the rigidity of a crystal structure may be due to the motion of its parts.

One curious observation which I have made is suggestive. I have many times noticed that when the appropriate crystal is introduced into a supersaturated solution, which is not strong enough to crystallize spontaneously, it may cause crystals to grow not only in actual contact with itself, but at some little distance in its neighbourhood. If this be so, then the crystallizing force, the power of propagating crystal growth, is not merely a frontier problem, but can be exercised through the liquid to a distance. If I try to picture to

myself what is happening, I must again have recourse to analogy; I can only think of the manner in which a string or a tuning-fork is set in vibration and responds to a similar string or tuning-fork which is giving out its note at the other end of the room; and so is it not possible that the movements, whatever they are, vibrations or pulsations or regular oscillations of some sort, which constitute crystalline growth may be communicated through the almost crystallizing liquid, and culminate at some point where they set similar material vibrating, that is to say, crystallizing, in the same way?

I remember, when I first began to be interested in crystals in undergraduate days, reading in *Nàture* an account of two papers which seemed to have a possible bearing on the subject. One is an example of a stationary arrangement of rigid bodies. The other illustrates the principle which I am now suggesting, and is an arrangement of pulsating bodies whose positions are due to their movements. The first was a description of MAYER'S experiment on floating magnets which was, if I remember right, shown here in a BOYLE Lecture by Sir JOSEPH THOMSON a few years ago; an experiment in which magnets suspended in water by corks and with their North poles projecting so that they repel one another, are brought together by the attraction of a large magnet held, with its South pole downwards, above the surface of the water; under the joint influence of this attraction and their own mutual repulsions, they group themselves into a number of interesting geometrical figures very suggestive of the geometrical regularity of crystal structure. Indeed, attempts have been made by LEHMANN to explain the architecture of

a crystal by a grouping of magnetic systems. And the other was the experiment of Bjerknes in 1876 (described in *Nature* in 1881), in which a number of hollow elastic balls were made to expand and contract by means of air tubes attached to them. These pulsating balls, when placed in water, attracted and repelled one another like magnets, and arranged themselves in a regular manner; thus suggesting that there may be many unexpected ways in which rhythmical motion can exercise an attractive and directive action such as is required to produce a crystal.

Let me illustrate what I mean by another crude analogy. Take a room full of dancers; if they are all dancing to different times and in different ways there will be no order or arrangement in the crowd, and it will remain an incoherent jostling assembly of independent persons. But if there are among them those who are moving to the same step and in the same manner, and if they come together, they can become partners and can continue to dance together; and if the room be filled with such dancers, then the whole assembly can grow together into an orderly movement, and only those whose step does not fit into the dance will be ejected and left out. Or take another example: soldiers marching together have not ceased to be individual men, but when they fell into step they became in addition an organized body with a structure and a coherence that does not belong to a miscellaneous crowd. Even so may the particles of dissolved salt be endowed with a movement which enables them to enter into partnership and cohere, and so to build themselves into the orderly structure which makes up a crystal. And even so do crystals grow out of a mixed solution as a pure

and homogeneous substance and reject the other materials which are dissolved in the liquid.

One other suggestion. If the growth of a crystal is really the coming together of vibrating particles which cohere because they are in tune with one another and so enter into a partnership like that of the dancers or the figure skaters, then is it not possible that we may be able to communicate these vibrations to a supersaturated solution, which is so densely crowded that it is ready to crystallize, by some other means than by inoculating it with the appropriate crystal? I think that the time has come when we may be able to get some knowledge of the manner of these movements by experimental methods; perhaps by studying the sort of shock or movement, if there be any such, which starts crystallization in a supersaturated solution; perhaps by finding other substances, whose movements we understand, which are able to start the crystallization when they are introduced into certain solutions.

I have said that the frontier problems in science are interesting; and that this is true not only of the events that take place when different bodies meet, but in another and more general sense. The most interesting and fruitful problems are those which deal with the borderland between two sciences, when the difficulties of the one are enlightened by the experience of the other, or when the same problem is looked at from two different sides. Let me only quote as an illustration the work connected with osmosis, which attracted first the botanists and then the physical chemists, and remind you of the enormous importance which it has assumed by their combined efforts. I think it will be found that these problems of crystal growth are of this

borderland nature. And such problems can only be satisfactorily attacked by those who are more than mere specialists and can be led by the experience or the analogies of other sciences.

I said that I should like to regard this lecture as an attempt to justify the value of analogy. We poor specialists grope within the confines of our own science, and what little advance we make is made in part by the light we borrow from the other sciences. In these days of increasing specialization what we need is to interest in our own problems, not only those who are tilling the same fields with ourselves, but even more, those who live on the other side of the fence which surrounds us: to call to them across the frontier, to seek their advice and assistance and the benefit of their experience.

This is one of the great advantages of a University, that workers and thinkers in different subjects are brought together and can make their difficulties known to each other. The connecting links between them are the analogies which they perceive and which excite an interest that they would not otherwise feel in each other's work.

This is the reason why I have endeavoured to set before you a very special problem in a very elementary way and as far as possible without using technical terms. It is with the hope of attracting the interest of workers in other subjects to this fascinating problem of crystal growth which, from the time of Boyle to that of Ruskin, and more especially, as I have shown you, in recent years, has engaged the attention of Oxford students of crystals.